

ROYAL ASTRONOMICAL SOCIETY.

VOL. V.

February 10, 1843.

No. 28.

Report of the Council of the Society to the Twenty-third Annual General Meeting, held this day.

In making this, the twenty-third, annual Report to the Members, the Council are happy in again announcing the progressive improvement of the Society, and the active steps taken to preserve its importance and utility: also in being able to announce that the expenses of the Society have been kept within the annual income, notwithstanding the cost of paper and printing, for the 12th volume of the *Memoirs* recently published, has been defrayed during the past year. Another volume (the 14th) is also now ready for delivery, and lies on the table; but the cost of printing this volume will not press so much on the funds of the Society, on account of the government having undertaken a portion of the expense, as will be more specially alluded to in the sequel of this Report. The 13th volume (which in point of order should have preceded this last-mentioned volume) is in a state of great forwardness, and will be finished in the course of the current year. So that within the space of 12 months no less than *three* volumes will have made their appearance, without encroaching much on the funds of the Society. The Report of the Auditors, which is here subjoined, will shew the state of the finances of the Society:

RECEIPTS.

	£.	s.	d.
Balance of last year's account	271	8	11
1 year's dividend on £900 Consols	26	4	4
1 year's ditto on £2000 3s. 1d. New 3½ per Cents	67	19	2
On account of arrears of contributions	48	6	0
70 annual contributions (1842-1843)	147	0	0
2 ditto (1843-1844)	4	4	0
3 compositions	63	0	0

Carried over..... £628 2 5

RECEIPTS (*continued*).

	£.	s.	d.
Brought forward.....	628	2	5
10 admission fees	21	0	0
7 first year's contributions	11	11	0
Sale of Memoirs, by the Assistant-Secretary, from Jan. 29, 1842, to Jan. 27, 1843	45	3	0
Ditto by J. Weale, from March 1840, to Dec. 1842	42	5	8
Balance of the fund, granted by the Treasury, for the Cavendish experiment	98	5	8
	<u>£846</u>	<u>7</u>	<u>9</u>

EXPENDITURE.

J. Basire, for engraving and printing plates 1-6, Vol. XII.	34	18	9	
Purchase of £20 18s. New 3½ per Cents	21	0	0	
W. Magnay, for 50 reams of paper	45	2	0	
J. Bagg for drawings and engravings for Cavendish experiment	10	15	0	
J. H. Thompson for ditto	2	0	0	
Moyes and Barclay, for printing Monthly Notices, Vol. V. Nos. 17 to 24	38	7	0	
Ditto, for printing Memoirs, Vol. XII.	239	11	0	
1 year's salary to the assistant-secretary	80	0	0	
Harrison and Co. for stationery.....	10	3	10	
Assistant-Secretary, for commission on collecting £277 4s.	13	17	3	
W. Rumfit, for binding books, &c.	1	4	3	
Charges on books, and carriage of parcels	3	10	0	
Postage of letters	15	5	10	
Porter's and charwoman's work, &c.	7	13	6	
Tea, sugar, cakes, &c. for the evening meetings	13	13	0	
Coals, candles, &c.	12	2	0	
Sundry disbursements by the treasurer	16	13	3	
Taxes { Poor's rate	6	3	3	
Church and rector's rate	3	16	6	
Sewer's rate	0	12	9	
Land tax	3	2	6	
Window duty	5	4	9	
		18	19	9
Balance in the hands of the treasurer (Jan. 28, 1843)	261	11	4	
	£846	7	9	

The assets and present property of the Society are as follow :

	£.	s.	d.
Balance in the hands of the Treasurer	261	11	4
3 contributions of four years' standing	£25	4	0
5 ——— of three ditto	31	10	0
17 ——— of two ditto	71	8	0
39 ——— of one ditto	81	18	0
		<u>210</u>	<u>0</u> <u>0</u>
£2000 3s. 1d. 3½ per Cent Annuities } valued at	£2850	0	0
£900 3 per Cent Consols			

2 Gold Medals unappropriated.

Unsold Memoirs of the Society.

Various astronomical instruments, books, prints, &c.

The progress and present state of the Society, with respect to the number of its Fellows and Associates, may be seen from the following abstract, continued from the Report of last year, viz. :

	Compounded.	Annual Contributors.	Non-residents.	Patrons, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1842	93	131	84	6	314	35	349
Since elected	3	6	...	...	9	1	10
Deceased	-2	-6	-3	...	-11	...	-11
Resigned	...	-4	...	...	-4	...	-4
Removals	...	+3	-3	...	...	...	...
Expelled	...	-3	...	...	-3	...	-3
February 1843	94	127	78	6	305	36	341

With respect to the instruments of the Society, it may be proper to place on record the present state of them.

The *Harrison* clock,
 The *Owen* portable circle,
 The *Owen* quadruple portable sextant,
 The *Beaufoy* circle,
 The *Beaufoy* transit,
 The *Herschelian* 7-feet reflector,
 The *Greig* universal instrument,

are in the apartments of the Society.

The brass quadrant, said to be *Lacaille's*,
 is in the apartments of the Royal Society, for safe custody.

The Standard Scale

is in the possession, and under the care of the Astronomer Royal.

The *Cavendish* apparatus

at present remains in the possession of Mr. Baily.

The remainder of the instruments are lent, during the pleasure of the Society, to the several parties undermentioned : viz.

The *Fuller* theodolite, to the Lords of the Admiralty.

The *Beaufoy* clock,
 The two invariable pendulums, } to the Royal Society.

The *Lee* circle, to Lord Wrottesley.

The *Wollaston* telescope to Professor Schumacher.

The other *Beaufoy* clock, to Major-General Pasley, R.E.

The Council consider it here proper to announce to the Fellows, that a new arrangement has been made with respect to the sale of the Memoirs of the Society. Hitherto the sale has, from time to time, been undertaken by different publishers, whilst a

portion of them might also be obtained at the apartments of the Society; which has caused some confusion of accounts, and an uncertainty as to the place where they could be procured by the public. In order to avoid these difficulties, the Council have resolved that the only place for the sale of the Memoirs, both to the public and to the Fellows, shall in future be at the apartments of the Society, similarly to the plan adopted by many other societies; and the assistant-secretary is consequently empowered to conduct the sale, and to receive the produce of the same, agreeably to a plan laid down for his guidance. The Council trust that this arrangement will be economical as well as useful.

The Council regret that they have to lay before the meeting a list of a much greater number of deceased Members than on any former occasion: no less than eleven, during the past year, being removed by the hand of death; amongst whom are some of the earliest members of the Society. Although these sad events must always be a source of regret to the members, yet the Council trust that it will act as a stimulus to those who survive, to repair the loss which has been thus occasioned, and lead to new efforts for the promotion of the objects which it has in view.

The Earl of Macclesfield was one of the early members of this Society, and continued his connexion with us till the time of his death. He was the great-grandson of the first Earl of Macclesfield, who was President of the Royal Society in 1752; and who had established an astronomical observatory, fitted up with excellent instruments, at Shirburn Castle, near Oxford, which Bradley frequently visited. The observations made at Shirburn are now in the Savilian library at Oxford; and Bradley acknowledged his obligations to them in enabling him to complete his researches on nutation and refraction. The observatory does not now exist: but in the library, which is still in great preservation, and contains many valuable printed works, there is a large collection of original letters, from men of science, in the last century; amongst which the names of Oughtred, Flamsteed, the Gregorys, Barrow, Wallis, and Newton, frequently occur. The late Earl of Macclesfield, whose decease we are now recording, permitted the late Professor Rigaud to make a selection of such letters as he thought might be most interesting to the public, which have since been printed at the University press of Oxford, and at the expense of the delegates, in two volumes. Amongst these documents is the first letter which Flamsteed wrote to the President of the Royal Society, on Nov. 24, 1669, and which was not known to be still in existence prior to this discovery. Only a portion of it was printed in the *Philosophical Transactions*; and in the present publication there are yet certain portions omitted, which are not now considered to be interesting.

Mr. Smeaton was a civil engineer, and was descended from a brother of his celebrated namesake: he had been but a short time a fellow of the Society when he was removed by death.

Mr. Thomas Tulleys was the second son of Charles Tulleys, the

optician, in whose workshop he was brought up, and to whose business he succeeded, first in partnership with an elder brother, and afterwards alone, on the death of the latter.

Rear-admiral d'Urban has been dead some years, but the Council did not receive any news of his decease till very recently.

The Rev. Michael Ward had been a fellow of the Society for a long period. He was fond of astronomy, and possessed a small observatory.

Major-General Shrapnell was well known to military men as the inventor of the destructive shell which bears his name. He was for many years a fellow of the Society, though his period of active exertion was almost past before the Society was established.

Mr. James Moore French, chronometer-maker, at the Royal Exchange, was a zealous and successful artist, and on several occasions gained the prize given to the best of the chronometers which were tried at Greenwich.

Captain William Tucker, R.N. was introduced to the Society by his uncle, the late Mr. Frend, of whom an account appears in the last annual Report. He perished in November last, in the 48th year of his age, on the wreck of the unfortunate East Indiaman *Reliance*, which was lost off Boulogne. The circumstances of his death, and the devotion of his last moments, as narrated by survivors, to the performance of an act of humanity, created the strongest public sympathy. He had been almost all his life in active service, and particularly in cruising against the slave-trade, in which he had been remarkably successful; and he gained his commander's commission by a daring and prosperous attack upon a slaver of twice his force. He had been, previously to his death, in command of the *Iris* frigate, and senior officer on the Cape Coast station, and the failure of his health, which obliged him to return to England in a merchant-vessel, led to his unfortunate catastrophe.

Commander Michael Atwell Slater, R.N., was an officer who had gained distinguished reputation in the scientific branch of his profession; and was well known to many members of this Society for his zeal in the extensive surveys in which he was occasionally engaged. It was in one of these useful labours that he was unfortunately cut off in the prime of life, on the 2d of February in last year, by falling into the sea over the cliff called Holburn Head, on the eastern extremity of Scotland.

Mr. Innes of Aberdeen was well known to astronomers as a zealous calculator of eclipses, occultations, and tides; which occupied a considerable portion of his time. He was brought up as a watch and clock-maker: and although his professional gains were but small, yet by living very economically, he was enabled to collect together a valuable collection of books, which he has left behind him. He was a man of very mild temper and unassuming manners: and, after a very slow decay of health, died on the 22d of May, 1842.

The Council feel sure of the approbation of the Society at large

in the award of a Gold Medal to Mr. Baily, for his persevering and skilful management of, and complete success in, the repetition of the Cavendish experiment. The President has undertaken to explain in detail the grounds of this resolution, and to state to the meeting the more than usual obligation under which the Society has been laid by Mr. Baily's patient and sagacious proceedings. The publication of the 14th volume of the *Memoirs*, which is wholly devoted to an account of this experiment, renders any description, even of its general features, unnecessary in this place: but the Council cannot here refuse themselves the pleasure of recording their opinion, that in no instance whatever, since the foundation of the Society, has its medal been more worthily won, whether the result be looked at with respect to the skill and industry by which it was attained, or to the complete sufficiency of the Memoir in which it is promulgated.

While on this subject, it may further be stated that the 13th volume of the *Memoirs*, referred to in the last annual report as about to be presented to the Society by Mr. Baily, and containing the catalogues of Ptolemy, Ulugh Beigh, Tycho Brahé, Halley, and Hevelius, is now nearly completed, and nothing but the attention requisite for the Cavendish experiment has prevented it from being actually ready. Thus the Society receives, in the course of one year, two of the most valuable volumes of its *Memoirs*, both from the labour, and one at the expense, of the same Fellow, and that Fellow the one of all to whom the Society is most indebted, independently of these rich contributions.

In the course of the last year, the question has been started, whether it would not be advisable to alter the numerical typography now in use, and to return to the old method of forming the Arabic figures, in the manner still usually practised in handwriting. A committee appointed to consider this subject reported unanimously in favour of the alteration, and the Council have accordingly given directions that it shall be carried into effect in all the future publications of the Society. The printers have met the proposition with a readiness which deserves the thanks of the Society, the change involving, as it does, some trouble and expense. Fortunately, however, it has been found that though the old type has been almost entirely disused for many years, the punches necessary to recast it are still, of every size which the Society wants, in the hands of the type-founders. The Council strongly recommend the alteration to the fellows in their own private publications, as they are sure that the form now in use bears no comparison, as to distinctness and legibility, with that which it is proposed to restore.

During the past year, the trustees of the Radcliffe Observatory at Oxford have, for the first time, published the observations made at that establishment, in an octavo volume, containing the observations made in the year 1840. The director of that observatory, Mr. Johnson, is one of our most active members, and well known to us as the author of the excellent catalogue of southern stars, printed at the expense of the East India Company, and rewarded

by this Society with its Gold Medal in the year 1835. Mr. Johnson, conceiving that it would be desirable to confine his attention principally to a selected class of observations, determined to re-observe those stars in Groombridge's catalogue that are situate to the north of the zenith of his place. The volume, here mentioned, contains the first attempt of this kind: and in it we see the same marks of minute accuracy and scrupulous integrity that were so evident in his former publication. The Council trust that the publication will be continued in like manner from year to year, as it is only in this way that the progress of discovery can be rendered of essential and permanent advantage.

It has been mentioned at the preceding anniversaries of this Society that the British Association had appropriated funds for three very useful and important catalogues, which the Council are now happy to state have been completed. The first is an extension of the catalogue of this Society, published in the second volume of its *Memoirs*, with certain additional columns that will render it of greater value to the practical astronomer. The British Association have granted the funds requisite for the publication of this work, which will soon be sent to the press. The next two catalogues contain the reduction of the stars observed by Lacaille at the Cape of Good Hope, and of those observed by Lalande at the *Ecole Militaire* at Paris. These catalogues are finished, and the British Association propose to apply to government for a grant of the requisite funds for printing them.

The Council, while reviewing the subjects connected with astronomy which have been brought before the Society since the last anniversary meeting, beg particularly to call the attention of the meeting to the labours of Professor Henderson and of M. Hansen. It will be remembered that in the President's address, on delivering the Gold Medal to M. Bessel for his researches on the parallax of the double star 61 *Cygni*, honourable mention was made of the labours of Professor Henderson in a similar inquiry with respect to *α Centauri*, founded on the reduction of his own observations made with the mural circle at the Cape of Good Hope. This indefatigable astronomer has within the present year presented us with the result of a series of observations made at his request by Mr. Maclear expressly for the purpose; and which, extending as they do considerably beyond a year, or the time during which the parallax goes through all its changes, and averaging from eight to ten observations of the double altitude of each star in every month, will at least afford good ground for determining whether the problem of the parallax of this remarkable star is likely to be solved by meridian observations. But without entering into the question of the evidence offered by the observations, our thanks are certainly due to the untiring zeal of Professor Henderson in prosecuting this most important but too much neglected branch of astronomy, and in this expression the Council feel sure that the meeting will join.

The meeting will scarcely need to be reminded of the discovery of M. Hansen, to which allusion has been made, his letter having

been so recently read before the Society. The want of some general method of expressing the perturbations of a body moving in an ellipse, whose eccentricity and whose inclination to the ecliptic are not small, has been, as it were, the opprobrium of modern physical astronomy. The method which has hitherto been employed of dividing the orbit into several portions, calculating the differentials of the perturbations for the points of the orbit thus decided on, and then integrating them by mechanical quadratures, seems scarcely worthy of the present state of analytical science; and has put the patience of astronomers to the severest trials as often as the return of an interesting comet, such as Halley's or Encke's, has made necessary the rigorous computation of its disturbances by the larger planets. Still, such has been the difficulty of the problem, that up to the present time no person since Lagrange seems to have suggested or hoped for any means of removing it. M. Hansen has laid before us the result of his first trial in the case of the comet of Encke, and the comparison of his computed perturbations with those made by Encke by mechanical quadratures, proves the accuracy of the method as well as the easy application of it. We may be allowed to express a hope that we shall be soon in possession of the method itself, which we are thus far entitled to regard as a most brilliant conquest over one of the residual difficulties of physical astronomy. The Council cannot refrain from congratulating the meeting on the above proofs that the science which we especially cultivate is still advancing; that each year adds something to our stock of previous knowledge of the constitution of the universe, and that, too, of an importance that marks the zeal and the talent with which, both at home and abroad, preparations are making for the complete solution of the few most interesting problems which yet remain to us.

The Council have great pleasure in drawing the attention of the meeting to the very valuable present which the Society has recently received from Admiral Greig, one of our members, and a distinguished officer in the Russian navy. This consists of an altitude and azimuth instrument, by M. Reichenbach* of Munich. The diameter of the azimuth circle is 15, and of the altitude circle 12 inches. The divisions, which are upon silver, read to 4" of space by 4 verniers upon each circle. The instrument is one of the kind which admits of repetition both in the horizontal and vertical planes, and is furnished with two telescopes; the principal one resting in Ys attached to the azimuth index, and the other placed below the azimuth circle, according to the ordinary arrangement. But a peculiarity deserving especial notice is the manner in which the usual difficulty of observing near the zenith is obviated. A diagonal reflector, in this case a prism, directs the rays through one of the pivots of the transit axis, in which the diaphragm and eye-piece are consequently placed, and thus the observer remains

* In the *Notice* for January 1843, Article III. the instrument is erroneously stated to have been made by M. Ertel.

in an easy, unaltered position, whatever may be the altitude of the object observed. It is almost superfluous to add, that the graduation, axes, tangent screws, and other delicate parts of this instrument, exhibit all the proofs of care and skill for which the maker was so long celebrated.

The Council feel confident that the meeting will unite with them in a warm expression of their thanks to Admiral Greig, not only for his munificent present, but also for the proof he has thus given that, though at a distance, he still looks upon us and our proceedings with that interest and regard which are doubly grateful as coming from an absent friend.

*Titles of Papers read before the Society, between February
1842 and February 1843.*

1842.

Mar. 11. On an Instrument adapted for Observing Right Ascensions and Declinations of Stars independently of Time, accompanied by Drawings made with the Camera Lucida by Capt. Basil Hall, R.N. By M. Wettinger. Communicated, with a Letter of Description, to Sir J. F. W. Herschel, Bart., by Capt. Basil Hall, R.N.

Letter from Professor Henderson to the Secretary, dated Edinburgh, Jan. 31, 1842, on the Determination of the Parallax of α Centauri, by recent Observations made by Mr. Maclear at the Cape of Good Hope.

Positions of 78 Fixed Stars contained in the A. S. C., represented by Mr. Baily as not determined with sufficient accuracy, deduced from Observations made with the Meridian Circle of the Observatory of Kremsmünster. By M. Köller, Director of the Observatory.

Observations of Falling Stars made at Hereford on the night of Nov. 12, 1841. By Henry Lawson, Esq.

A List of Falling Stars observed Nov. 12, 1841, at St. Helena. By J. H. Lefroy, Esq. R.A. Director of the Magnetic Observatory at Longwood.

Path of the Moon's Shadow over the Southern part of France, the North of Italy, and part of Germany, during the Total Eclipse of the Sun on July 7, 1842. By Lieut. W. S. Stratford, R.N.

April 8. On the Aggregate Mass of the Binary Star 61 Cygni. By S. M. Drach, Esq.

Second Note on the Mass of Venus. By R. W. Rothman, Esq.

On a Method of determining the Latitude at Sea. By M. C. L. von Littrow, adjoint-Astronomer at the Imperial Observatory at Vienna. Communicated by the Rev. W. Whewell, Master of Trinity College, Cambridge.

1842.

- April 8. On the Rectification of Equatorials by Observations of Stars on the Meridian, and at an Hour-Angle of Six Hours. By M. C. L. von Littrow. Communicated by the Rev. W. Whewell.

The Parallax of α Centauri, deduced from Mr. Maclear's Observations at the Cape of Good Hope, in the years 1839 and 1840. By Professor Henderson.

Observations of the Beginning and End of the Solar Eclipse of July 18, 1841. By Dr. Cruikshank. Communicated by G. Innes, Esq.

- May 13. Extract of a Letter from Professor Encke to F. Baily, Esq. on the Periodical Comet of Encke.

A Letter from Professor Chevallier, on some Phenomena observable in Total and Annular Solar Eclipses.

A verbal Account, by G. B. Airy, Esq., illustrated by a full-sized Model, of a Zenith Sector, then in the course of construction by Messrs. Troughton and Simms, under his direction, for the use of the Trigonometrical Survey.

- June 10. An Account of some Experiments with the Torsion Rod, for determining the Mean Density of the Earth. By Francis Baily, Esq. Vice-President of the Society.

- Nov. 11. A few Remarks on the Total Eclipse of the Sun, observed at Nice, July 8, 1842. By Capt. John Grover.

Observation of the Time of the Termination of the Solar Eclipse of July 8, 1842. By Arthur Utting, Esq. Communicated by E. Riddle, Esq.

Some Remarks on the Total Eclipse of the Sun, on July 8, 1842. By Francis Baily, Esq. Vice-President of this Society.

Observations of the Total Solar Eclipse of 1842, July 7 (July 8, civil reckoning). By G. B. Airy, Esq. Astronomer Royal.

An Explanation, by the Astronomer Royal, G. B. Airy, Esq., of the principle of an Escapement recently invented by him, and intended to be applied to a Clock made by Mr. Dent for the Observatory of Pulkowa.

- Dec. 9. A Letter from Professor Henderson to the Secretary, on the Parallaxes of certain Southern Stars.

Observations of the Beginning and End of the Solar Eclipse of July 7, 1842. Communicated by C. Rumker, Esq., in a letter to Dr. Lee.

Occultations observed at Yarmouth. By Arthur Utting, Esq. Communicated in a letter to E. Riddle, Esq.

Sequel to a paper "On a new Method for greatly Facilitating the Computation of the Moon's Co-ordinates." By S. M. Drach, Esq.

1843.

- Jan. 13. Translation of a Letter from Professor Hansen to G. B. Airy, Esq., the Astronomer Royal, on a New Method of Computing the Perturbations of Planets whose Eccentricities and Inclinations are not small. Communicated by G. B. Airy, Esq.

1843.

- Jan. 13. On a New Arrangement of a Vertical Collimator attached to the Altitude and Azimuth Instrument. By W. Simms, Esq.
- Description of a Universal Instrument made by M. Reichenbach of Munich, and presented to the Society by Alexis Greig, Esq. Vice-Admiral in the Imperial Russian Navy. By M. Ertel.
- Occultations observed, chiefly at Ashurst, in the year 1842. By R. Snow, Esq.
- Observations on the (apparently periodical) Variations in the Lustre of certain Stars of the First Magnitude. By T. Forster, Esq.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

American Philosophical Society.
 Society of Arts.
 Royal Asiatic Society.
 The Lords Commissioners of the Admiralty.
 Royal Academy of Berlin.
 British Association.
 Royal Academy of Sciences, Brussels.
 Cambridge Philosophical Society.
 Editor of the Athenæum Journal.
 Royal Society of Edinburgh.
 L'Académie Royale des Sciences de l'Institut de France.
 Royal Geographical Society.
 Geological Society of London.
 Imperial Academy of Sciences at St. Petersburg.
 Her Majesty's Government.
 Meteorological Society.
 Linnean Society.
 The Editor of the Microscopic Journal.
 L'Administration des Mines de Russie.
 The Observatory at Vilna.
 The Royal Society of London.
 The Radcliffe Trustees.
 The Editor of the Polytechnic Review.
 Zoological Society.
 Dépôt de la Marine de France.
 The Hon. Board of Ordnance.
 The Newcastle Philosophical Society.
 The Literary and Philosophical Society of Manchester.
 Society of Geneva.

M. Argelander.
 Professor Bessel.
 A. De Morgan, Esq.

E. J. Dent, Esq.
 M. De Vico.
 M. Dorn.

Professor Encke.
 T. Forster, Esq.
 Admiral Greig.
 J. Herapath, Esq.
 Professor Hassler.
 G. Innes, Esq.
 J. Lockhart, Esq.
 M. Leverrier.
 M. Nobile.
 Professor Quetelet.

W. Rutherford, Esq.
 Lieut. Raper, R.N.
 The Rev. T. R. Robinson.
 Lieut. Stratford, R.N.
 R. Snow, Esq.
 M. Struve.
 M. Simonoff.
 Professor Schumacher.
 R. Taylor, Esq.

The President (Lord Wrottesley) then addressed the Meeting on the subject of the award of the Medal, as follows :

Gentlemen,—I have now the gratifying duty of stating to this meeting the grounds on which the Council have thought it right to award a Gold Medal to Mr. Francis Baily for his experiments to determine the mean density of the earth, in repetition of what is generally termed the *Cavendish experiment*. In the performance of this duty, I am necessarily required to offer a few remarks on the nature and utility of the end sought, on the previous attempts which had been made to gain it, and on the manner in which the one now before us was conducted by the distinguished friend of the Society to whom we are this day to offer our highest token of acknowledgment.

The labours of the astronomer are directed not only to the accurate determination of the motions of the heavenly bodies, but also to that of their constitution and organisation. If the papers in our *Memoirs* and those of other kindred societies seem to dwell much upon the former division of the subject, and little upon the latter, it is not of preference, but of necessity. Our means of determining satisfactorily any thing which relates to the constitution, even of the bodies of our own system, are few and limited: while those which apply to the prediction of their relative motions constitute the most perfect body of science which exists. But all which is known, certainly or conjecturally, of the interior arrangements of the heavenly bodies, is of the highest interest, and most especially when its action upon the minds of men is considered. Those who have heard the son and successor of the patriarch of this branch of astronomy, on occasions similar to the present one, deliver his views upon the general constitution of planetary systems, cannot but have felt that the subject which could inspire such thoughts must, were it for that reason only, be among the noblest to which human energy can be directed.

The masses of the planetary bodies are important data of the Newtonian theory of gravitation, but only in a relative sense. If, at any given instant, each one of the innumerable particles of which the universe is composed were to acquire a doubly attractive power, and, at the same moment, a double resistance to the alteration of

its state, the effect of the change would be unseen and unfelt, so far as the motions of the system are concerned. Nothing, therefore, is needed, in this last point of view, except a knowledge of the relative quantities of matter contained in the different planets.

There is something vague at the outset in the term *mass* or *quantity of matter*. With this the calculator of the planetary motions has nothing to do: the term *mass* is to him merely a convenient name for the numerator of the fraction which expresses the attractive force of that planet; and what number stands in any one numerator is of no consequence whatever, provided that all the other numerators are in their proper proportion to that one. Hence the mass of the earth, for instance, may be called unity, and the results of observation may be made to give the relative masses of other bodies.

What, then, is the mechanical signification of this numerator? The answer to this question belongs to the physical astronomer, properly so called.

Our first idea of mass is derived from bodies at the surface of the earth; under the same bulk some are lighter, others heavier. Two explanations present themselves: either the heavier body is composed of matter of the same kind as the lighter, but in a state of greater density, or more closely packed; or else the heavier body is composed of particles intrinsically heavier, that is, more powerfully acted on by the earth than those of the lighter. Which of these two explanations is to be received as the true one, more concerns the chemist than the astronomer, but the language of the former explanation is always used in our science. If we have reason to know that water compressed into one-twentieth of its bulk would produce all the mechanical effects of gold, then the latter is, and must be, in mechanics, considered as containing twenty times the quantity of matter of the former, under a given volume.

We are in the habit of referring solid bodies to water, in estimating their quantities of matter, and the specific gravities of the several kinds of matter, or the ratios of the weights of given bulks to the weight of the same bulk of water, have been carefully determined. Now among the questions of primary interest to the physical astronomer comes the following: — What is the quantity of matter in the whole earth, from which that in the several other planets is so easily determined? If the earth were a ball of water, what alteration would need to be made in the numerator of the fraction which expresses its attractive power? Both these questions are answered together; and it is only within the last seventy years that any attempt at an answer has been made on rational grounds.

If we knew the materials of which the earth is composed, it would be a mere matter of calculation to answer the preceding questions. But, ignorant as we are, *à priori*, even as to the fact whether there is an interior to the earth at all, that is, whether it is a solid globe or only a hollow shell, we cannot possibly be in possession of the means of forming so much as a guess at the

character of the answer. There is but one mode of proceeding, and that was, no doubt, suggested by the processes and results of the theory of gravitation. Since a comparison of the actions of two planets upon a third can be made to give the ratio of the masses of those planets, it is obvious that if we can compare the effect of the whole earth with the effect of any known part of it, we may deduce a comparison of the mass of the whole earth with the mass of that part of it. A mountain in one case, a large leaden ball in another, have been weighed against the whole earth by comparison of their effects upon a pendulum: the nearness of the smaller mass making it produce a sensible effect as compared with that of the larger; for by the known laws of attraction the whole earth must be considered as collected at its centre.

Before giving any account of the different modes pursued, it may be permitted to pause a moment, and to consider the bearings of the result. Independently of the satisfaction which the mind receives from the conversion of relative into absolute knowledge, independently of the stimulus given to the advance of inquiry by the acquisition of such a resting point as the determination before us, and of the value which it adds to the theory of gravitation as an instructor of the world at large in the laws of nature, by enabling the teacher to present what would have been a mathematical conception in a more physical and tangible form—there are consequences of no mean importance which spring from the comparison of the whole earth with one of its definite parts.

I do not undervalue considerations which present themselves at every step which is made, at every height which is scaled, when I confine myself on this occasion, and before this Society, to those only which particularly concern just knowledge of the foundations of astronomy, and fitting preparation for its advance among the physical sciences.

When the *Principia* was published, one of the first who gave an unqualified adhesion to the general views of Newton was the justly celebrated Huyghens.* But there was one point which he could not bring himself to admit; it was the universal attraction of every particle upon every other. He could not feel certain that, because the attraction of the whole of one planet upon the whole of every other was established, it followed that the parts of each attracted the parts of all the rest and of each other. Newton did not, and could not, make this important conclusion a fundamental experimental fact, though he was able to advance almost an overpowering presumption in its favour: and it is easy to see how the student of Descartes, even when he had been brought to admit the attraction of planet upon planet, might have suspended his opinion as to the action of part upon part, until the cause of the phenomenon, an inquiry much agitated in those days, was settled. But though the strong balance of probabilities in favour of Newton's opinion gradually gained for it a universal reception, there was no

* Hugenii Opera Reliqua, vol. i. p. 116.

ocular and crucial evidence for the physical fact until the experiments were made which I shall presently describe; and of those experiments, the one of Cavendish, which Mr. Baily has repeated, was by far the most convincing. Seeing is believing; and no one acquainted with the apparatus, and actually noting the visible effect of the approach of a leaden ball upon the oscillations of a torsion pendulum, after the nicest precautions had been taken to remove any possible effect of currents, magnetism, electricity, and heat, could doubt the presence of a totally distinct agent, producing the effect of attraction, and transmitting its influence through all the screens which were employed to exclude disturbing causes, with as much ease as through space occupied only by air.

It had been previously shewn by Laplace, that the mean density of the earth must exceed that of the sea, in order to preserve the stability of the oscillations of the latter: and subsequently, Poisson, in a paper contained in the seventh volume of the *Memoirs of the Institute*, concludes (taking Cavendish's value of the mean density as one of the data, and applying the forces with which the sun and moon act in causing precession, in a manner which is perfectly just, though open to a small amount of objection against the numerical accuracy of the remaining data) 'not only that there must be an increase of density from the circumference towards the centre, but that, even on the supposition of an homogeneous nucleus, the variable strata which enwrap this nucleus must extend to a depth of at least one-fourth of the radius. The physical astronomer, using that power of judging which the study of past discovery has made habitual to him, has long considered it as all but proved that our terrestrial globe is not only solid to the centre, or at least having its interior strata composed of fluid with the density of a solid, but also that the density of successive strata is gradually increasing from the surface to the centre itself. But those who have not studied physics, and even experimental philosophers of note who have not paid particular attention to the theory of gravitation, have frequently taken the hypothesis of an hollow globe as being that which has the highest probability in its favour. The late Professor Leslie, for example, takes the hollowness of our earth for granted, and proceeds to reason upon the manner in which the part of the interior which is destitute of solid matter is filled. It is his opinion that it is a sure result of induction,* that "the great central concavity is not that dark and dreary abyss which the fancy of poets had pictured. On the contrary, this spacious internal vault must contain the purest ethereal essence, *Light* in its most concentrated state, shining with intense refulgence and overpowering splendour." It is not my intention to discuss the grounds upon which, taking comparative hollowness for granted, the void is to be supposed filled with light or any other ether, but only to observe that the experiment before us strikes most completely at the preliminary assumption. Maskelyne made the earth to be a

* *Elements of Natural Philosophy*, vol. i. p. 453.

ball five times as dense as the same bulk of water; Cavendish, five times and a half; but Mr. Baily, whose experiments are far more numerous and well supported, five times and two-thirds. The solid rocks with which the geologist professes that his knowledge of the interior strata of our earth terminates, are only between three and four times as heavy as water: barytes itself, which takes its name from the great density of its compounds, has one of the heaviest of those compounds, the sulphate, under four times and a half its weight of water. If we were to ask what substance must be chosen, so that a globe uniformly consisting of that substance might take the place of our earth in the planetary system, retaining its size, the answer would be that nothing under the ores of silver or the lighter ores of lead would serve the purpose. The increase of density, then, from the surface towards the centre, is fully confirmed by this experiment: a highly probable result of the theory of gravitation is made as sure as any result can be. But while we thus admire the manner in which one inquiry is made to confirm the consequences of another, we must not forget that it is no small part of our province to apply the conclusions of sound knowledge to the destruction of those remains of the age of speculation which yet linger about the porch of inductive philosophy; and at which those who have safely gained the inner court will feel little temptation to laugh, when they remember how many may be, and probably are, led finally away by the delusions which lie in wait at the entrance. Among these the assumption that the earth is a mere hollow shell has held a conspicuous place: let us take this assumption, and grant that the depth of the solid matter is even one-eighth of the whole radius, which is more than many speculators would admit. To make such a supposition consistent with the result of the experiment before us, it must be inferred that the actual matter of the shell considerably exceeds mercury in mean density, and is all but equal to hammered gold. Let such a result be established, if it be possible; but in the meantime, and until presumption can be shewn in its favour, let it be the office of the experiment before us to check the wildness of mere hypothesis.

The French academicians, in measuring their South American degree, were the first who found sufficient local attraction in a mountain adjacent to their observations (Chimborazo) to give any hope of making that phenomenon useful in future inquiry. Maskelyne, in 1772, suggested the employment of astronomical observation in the neighbourhood of a mountain, for the determination of the earth's mean density. Schehallien was chosen for the purpose by a committee of the Royal Society, and the result of this celebrated experiment was announced in 1775. The description of this purely statical experiment is easier than in that of Cavendish: the position of a plumb-line, in a state of deviation from the vertical of the place, on account of the attraction of a mountain, is first to be accurately determined by making that plumb-line the regulator of an instrument for measuring zenith distances, and comparing the zenith distances thus obtained with those determined in other

places of known differences of latitude. The weight of the plumb-line is then acted on by three forces of known direction, one of them being of known magnitude. The remaining forces (one of which is the attraction of the mountain) can then be determined: so that if the distance of the mountain be known, its mass can be compared with that of the whole earth. The next step, and the most difficult one, is to compare the mean density of the mountain with that of water, which requires an accurate knowledge of its material and size. Maskelyne's rough computation, made with such knowledge as he had of the composition of the mountain, gave the mean density of the earth nine-fifths of that of the mountain, or from four to five times that of water; Hutton's subsequent and laborious investigation made four and a half for the ratio; but Playfair's examination of the strata of the mountain led to the inference that the result of the experiment could only be considered as placing the same ratio between four and six-tenths and four and nine-tenths. It is worth noting that Newton (*Princ.* book iii. prop. 10) had ventured a conjecture which, as happens so frequently with him, turns out to be true. He thinks that the mean density of the earth is *between five and six times that of water*.

The method first proposed by Michell, and adopted and executed by Cavendish, was wholly independent both of astronomical data and of the uncertainty of the material of comparison. A horizontal pendulum, suspended by a wire, the torsion of which caused it to make slight oscillations about a position of equilibrium, was substituted for the gravitation pendulum, or plumb-line; while a massive leaden ball took the place of the mountain. If the torsion pendulum had been as secure of its position of equilibrium as that of gravitation, the experiment would have been almost as identical in its details with that of Maskelyne as it is in its principle. The time of an oscillation would first have led to the settlement of the amount of torsion force in any given position of the pendulum: it is well known that nothing but the time of oscillation is wanted to give the means of determining the quantity of restitutive force which acts on the pendulum at any degree of departure from equilibrium. The degree of departure caused by submitting the ball at the end of the torsion pendulum to the lateral influence of the large mass would then have given the means of calculating the attraction of that mass, just as the amount of deviation of the plumb-line gave that of the mountain in Maskelyne's experiment. Small as may be the leaden mass compared with the mountain, it was so much better known as to make the risk of error materially less; to which must be added, that it was submitted to an instrument of very much greater delicacy than the ordinary plumb-line.

The torsion pendulum is, in fact, possessed of such an extreme susceptibility, that every detail of the experiment requires adaptation, to an extent which would make a superficial inquirer wonder how it could be in any way compared with that of the plumb-line and mountain. There is no position of equilibrium — the instru-

ment is never at rest. The effect of presenting the large leaden ball is not to draw the torsion-rod from one position into another, but to change its motion from an oscillation of one extent about one point of rest to one of another extent about another point of rest; and a careful and peculiar mode (into which I cannot here enter) must be practised of determining this point of rest, both before and after the pendulum is placed under the influence of the leaden mass. This point of rest, so called, is not at rest, and resembles one of the elements of an elliptic orbit, which would remain constant if there were no foreign disturbance, but is perpetually changing from the action of other planets. And so fast do the indications of the pendulum vary, that is, so rapidly does it change the character of its oscillation, even where no visible cause is at work, that it cannot be permitted to take a long series of observations to determine either the point of rest which precedes the change of position of the attracting mass, or that which immediately follows. For the instrument is always in a state of change, in a manner which would lead to the supposition that the torsion of the suspending wire is either visibly influenced by invisible changes of temperature, &c., or that it is acted upon by some agent of a character wholly unknown.

From seventeen experiments, Cavendish, in 1797, deduced 5.45 for the mean density of the earth; and nothing further was done in this species of experiment until 1836, when it was repeated by M. Reich, of Freiberg, who followed Cavendish's plan in every particular, except in having one leaden mass instead of two. From fifty-seven experiments, M. Reich deduced 5.44 for the mean density of the earth,—a result almost identical with that of Cavendish.

In the year 1835, the Council of this Society appointed a Committee to consider of the best mode of procuring a repetition of the experiment. After some delay, occasioned by the difficulty of procuring funds, and choosing a site, the construction of the necessary apparatus was commenced at the end of 1837. On a representation made by the Astronomer Royal, the Government, in the year just named, granted £500 for the purpose; and Mr. Baily (to whom the conduct of the whole was intrusted) chose, after much deliberation, to make his own house the scene of operation. About £400 has been expended in the actual experiment, and it is right here to acknowledge the liberality of the present Government, which has sanctioned the application of £100 remaining out of the sum granted by the late Government, to the payment of part of the expense of printing the results.

I could hardly undertake to make a description of the apparatus intelligible in little time, or without diagrams; but fortunately this attempt is rendered unnecessary by the fact of an abstract of Mr. Baily's paper having been for some months in the hands of the fellows of the Society. Keeping in view, then, the main point of the present address, I shall make some remarks upon the progress of the experiment, with reference to the justification of the decision

of the Council, on which it will presently be my duty to present a gold medal to Mr. Baily.

And first, with regard to the responsibility which was imposed. It ought to be most distinctly understood, that the functions of the Committee appointed by the Council ceased as soon as the money was obtained for the expenses, and Mr. Baily's offer of superintendence was accepted. Not one direction was given, not one single condition was imposed, not one syllable was put upon record by which failure, had it occurred, could have been thrown from the shoulders of one upon those of several. The suggestions which Mr. Baily acknowledges himself to have received from men of science, whether in or out of the Society, were submitted solely to his judgment, as much as if the experiment had been his own private undertaking. Consequently, it is just that it should be most explicitly acknowledged that all the merit of success lies where all the blame of failure would have fallen. But, while I say this, I must also at the same time observe that the disposition to ask advice, and to try advice, which has characterised Mr. Baily's progress throughout, has been in no small degree conducive to the distinguished character of the result. The Council, when they placed in his hands, and at his sole disposal, the public money with which they had been intrusted, were well aware that no pains would be spared to collect, to compare, and to choose among, the best opinions which could be obtained. And, while on this subject, it is proper to acknowledge the obligations of the Society to Professor Forbes, for the suggestion which overcame the difficulty, and nearly destroyed the anomalies of the torsion pendulum, and to the Astronomer Royal, for his paper on the mathematical theory of Cavendish's Experiment, which will appear as a part of Mr. Baily's *Memoir*.

In the next place, it is necessary, before proceeding to the business of the day, to separate most emphatically the conductor of the experiment from the able and energetic friend of the Society in other respects. If an endowment had been bequeathed for the purpose of enabling the Council to give a yearly medal to the Member who should have been most active in carrying on the ordinary and extraordinary business of the Society, who is there that hears me, I speak particularly to those who are, or have been, on the Council,—who could positively undertake to say that such a medal could, up to this time, have been gained by any one except Mr. Baily? No doubt we have many among us whose active services the Society must most gratefully acknowledge:—our records prove that fact: indeed, I think I may venture to say that ours has been a fortunate one among societies in the amounts of service rendered by individuals. No doubt, again, that the honourable leisure gained by a long attention to business has placed in Mr. Baily's hands a power of serving the Society which most of the Fellows do not possess. But this does not alter the fact, that he has been, ever since its foundation, identified with its progress, and assisting its efforts, in a greater degree than any

other individual member. I state this now, not to acknowledge services which are so perpetually before the minds of all who take an interest in us, but to request you, if you can, to forget them for a short time, and to look on the experiment before us as if it had been the work of a new man, hitherto unknown to the Society, and resting his claim to our gratitude wholly and solely upon its conduct and its result. It is only thus that you can fairly affirm the verdict which the Council has already given, and the approval of which it so confidently expects at your hands; for you must remember, gentlemen, that no services whatever, except those expressed in the resolution awarding this medal, can count in the smallest degree as a justification of that resolution. I am the more desirous of impressing this upon you, when I consider that this is the first occasion on which a medal has been awarded for the manner of employing money intrusted by the public to our charge: so that in fact, this defence, if I may so call it, of the award, is more than the explanation of the Council to their constituents, or at least must become something more. When it has gained your approbation, it must go forth to the public, and to the government, as the account which the Society renders of the funds which were placed in its hands, as the proof that those funds were worthily administered. I am not, of course, alluding to the mere vouchers of pecuniary integrity, to the proof that money asserted to have been spent upon apparatus was actually so expended, but to the justification of a yet higher character.

The actual observations, meaning those which are printed in the *Memoir*, are 2153 in number, varying from ten to thirty minutes each; so that I am under the mark considerably when I say that 600 hours were spent at the apparatus, in the mere act of watching the oscillations of the torsion rod. To this must be added nearly as many more in the series of experiments, of which the results were afterwards abandoned on account of the anomalies of the pendulum. Add to this the time expended in contrivance, in computation, and in deliberation, and it will appear that it is not often that any single inquiry has called forth so determined an exercise of industry and perseverance. The experiments were commenced in October 1838, and were continued until May 1842. With the exception of about a couple of months of interruption, caused by the severe accident which, as all present are aware, happened to Mr. Baily in the summer of 1841, there was a continued succession of trials. This long and patient investigation gives a peculiar value to the result: the effects of the several conditions of the atmosphere cannot but have been eliminated, since the *printed* experiments, or those which were made *after the anomalies of the pendulum were got rid of*, run over nearly a whole year.

I cannot but call your attention to the spirit in which every part of the investigation was conducted. When all possible precautions had been taken to secure the stability of the pendulum; when it had been ascertained that no degree of concussion, whether in the room which contained the apparatus, or in that immediately

above, would produce any sensible effect upon it ; when every pains had been taken to remove thermal, electric, or magnetic disturbance ; and at the moment when it seemed next to certain that the result, whether that of Cavendish or another, was about to come out clearly, easily, and honestly, it was found that the work was yet to begin ; that, in fact, the torsion pendulum was subject to every species of anomalous motion. In vain were such new precautions taken as these appearances suggested, and, for eighteen months, a period so easy to speak of, so difficult to employ in activity under discouragement, no clue appeared to the solution of phenomena which seemed to set all the known laws of mechanics at defiance. Nothing could be more even than the temperature, even at the times when the anomalies were greatest ; so that the effects of heat seemed to be out of the question as an explanation. The torsion rod, secure in a wooden case with thick glass ends, seemed to bid defiance to all external cause of currents of air ; and so completely was the pendulum isolated, even from the case itself, that the latter might be violently shaken without any perceptible effect upon the former. Cavendish and Reich had both observed corresponding anomalies, but both apparently considered that their effects would disappear in the mean of a large number of observations. Mr. Baily resolved not to quit the subject until the cause of the anomalies was detected, and upon no account whatsoever to present a result vitiated either by assumption of the nature of the difficulty, or by rejection of the observations which appeared most discordant. To this resolute and honest determination we owe it that the paper for which this medal is given to-day is hardly less valuable as a lesson upon the nature and use of the torsion pendulum in measuring small forces than as a determination of the mean density of the earth.

It was at last suggested, and, as I have before stated, by Professor Forbes, that possibly the radiation of heat from the large masses might, when they were brought up close to the torsion box, or case of the pendulum, affect the inside of that case. It was already known that the evaporation of a few drops of spirits of wine sprinkled on the side of the case would produce a large and rapid effect on the pendulum. But, between this frame and the large masses there had already been interposed a wooden screen, which, it was thought, would wholly prevent any effect of radiation. The suggestion above mentioned was accompanied by a recommendation that the outside of the case, and the masses themselves, should be gilt. Mr. Baily carried his precautions still further : the case was first wrapped in flannel, over which a new case (gilt) was placed ; the masses, the plank which carried them, and the interior of the frame-work which inclosed both torsion box and masses, were covered with gilt paper ; the leaden balls at the two extremities of the torsion pendulum were gilt and burnished ; and the masses were made to stand, when at the nearest, a little further from the torsion box. These precautions proved to be sufficient ; the anomalies were substantially removed, shewing themselves only now

and then, and in smaller quantities. The lesson thus read to experimentalists on the effects of radiant heat will, it may be hoped, lead to further inquiries. Mere creation of difference of temperature was insufficient to point out the source of the anomaly: red-hot balls, lamps, or lumps of ice, placed near the torsion box, failed to give any clue to the cause of the difficulty.

All the observations, good, bad, or indifferent, made since the removal of the effect of radiation, have been taken into account in the general mean. And they have all been printed at full length, in such a manner that any one can go through the whole of the calculation in every experiment, from the announcement of one of the raw data of observation at the telescope, to the deduction of the mean density of the earth. Thus, as no experiment was ever more honestly or diligently performed, so also none has ever been more completely or satisfactorily described.

Now as to the result. The mean of all the experiments gives 5.675 as the mean density of the earth, with a probable error of .004. The balls used at the ends of the torsion pendulum have been lead of different sizes, brass, platina, zinc, glass, and ivory; and in some of the experiments, a torsion rod of brass, without any balls, has been used. Various modes of suspension have been employed, single copper wire, double iron, double brass, and double silk thread. The discordances which occur between the results of different balls, modes of suspension, or both, have every appearance of being the consequence of an insufficient knowledge of the torsion pendulum, and lend no countenance whatever to the suspicion that the attraction of matter upon matter varies in different substances. A moderate examination will shew that there is no doubt that the discordances, being such as might have been looked for in any inquiry, must disappear in the mean of so large a number of observations.

We may, then, confidently assert that this important element of the physical part of astronomy is settled, within very narrow limits of error. But suppose, if possible, that a less degree of trust were to be accorded to the mere result: nay, go further, and imagine the theory of gravitation itself, the best demonstrated of all general laws, to be an unfounded delusion. Perhaps we are then, on such a supposition, to give a still higher degree of praise to the manner in which this inquiry has been conducted. All the experiments are published, and all the experiments are *facts*. I have no doubt, and those who hear me have no doubt, that attraction is as real an existence in physics as it is an explanatory hypothesis in mathematics: and experiments of the nature of that before us seem to me, as to you, to put this beyond doubt, both in the hands of Maskelyne, Cavendish, Reich, and Baily. But if we be wrong, how shall we ever be brought to know our error? How, except by experiments conducted with that firm honesty of purpose, and true absence of all bias, which has characterised those described by me to-day. I repeat that these experiments are facts, facts which are and will be true, facts which are the result of an inquiry in which the Newtonian

doctrine was fairly thrown into the scale, and weighed by Nature's own weights. The confirmation of the general truth of Cavendish's mean density of the earth is the *numerical* result: but one more assurance that a century of patient and truth-loving inquiry will not fail of its reward, is the *moral* result.

It generally happens that in the award of our honorary distinctions, our immediate interest is limited to that which we must always take in an extension of our science, whether of principle, of process, or of fact; from what country or quarter soever it may come. We always acknowledge an addition to astronomy as a claim upon our gratitude: how particularly then is it incumbent upon us in the present instance, when the character of the Society depended upon the fulfilment of the pledges under which the means of making the experiment were obtained. Had that experiment failed, had it shewn that Cavendish had deceived himself when he thought of obtaining the earth's mean density by his now established mode, there might indeed have been regret, but there would have been no shame. In any human undertaking no censure need be justly feared, when it can be shewn that extreme diligence, unshrinking honesty, long deliberation, and the most candid research after, and use of, the suggestions and advice of others, have marked its progress from the beginning to the end. But when to all this we have a right to add success—when we can feel that we now present to the philosophical world a result on which they may confidently rely, knowing that the history of the experiment bears evidence which cannot be mistaken of its intrinsic value—we must consider this medal a token of what I may venture to call the *personal* gratitude of the Society towards one of its body who feared neither responsibility nor toil in its cause.

When, at some future time, those who are to profit by the labours of our day shall, with improved instruments and extended knowledge, once more repeat the interesting experiment to which these remarks refer, they will find in the records of this attempt proofs of its honesty which are now hidden from us by those very instrumental deficiencies and theoretical imperfections, the removal of which will be the signal to renew the process. And, in like manner as we now render due honour to Cavendish, not only for the first actual performance of the experiment itself, but because, with comparatively rude apparatus and few trials, he came so near the truth of which he was in search, so will they remember to celebrate the patience, the integrity, and the sagacity of the philosopher who made the next step, and who shewed them the path of amelioration. I cannot better express my strong feeling for the honour and welfare of this Society, than by claiming your response to an earnest wish and desire that, when that time shall come, our Fellows may be among the foremost promoters of the revival of this experiment, and that they may find among themselves one to whom they dare intrust the sole superintendence, and who will justify their confidence as well as Mr. Baily has done that of your Council on the present occasion.

The President then, addressing Mr. Baily, continued as follows :

Mr. Baily,—I present you with this medal in the name of the Society for which you have done so much, as the highest testimony which they have power to bear to the splendid service which you have rendered to the science of Astronomy. I thank you in their name for the care which you have taken of the honour of the Society, and for the augmentation which it has received from your labours. And to our sincere congratulations upon the providential escape which you experienced, during the prosecution of your inquiry, from a sudden and violent death, I add the expression of our earnest hope that you may long be spared to continue your services in the advancement of knowledge, and to enjoy that well-earned fame which will wait upon your life and your memory.

The Meeting then proceeded to the Election of the Council for the ensuing Year, when the following Fellows were elected, viz. :

President : Francis Baily, Esq., F.R.S.—*Vice-Presidents :* George Biddell Airy, Esq., M.A. F.R.S., *Astronomer Royal*; Augustus De Morgan, Esq.; Rev. George Fisher, M.A. F.R.S.; Lord Wrottesley, M.A. F.R.S.—*Treasurer :* George Bishop, Esq.—*Secretaries :* Thomas Galloway, Esq., M.A. F.R.S.; Rev. Robert Main, M.A.—*Foreign Secretary :* Captain W. H. Smyth, R.N. K.S.F. D.C.L. F.R.S.—*Council :* Samuel H. Christie, Esq., M.A. F.R.S.; Rev. W. Rutter Dawes; Thomas Jones, Esq., F.R.S.; John Lee, Esq., LL.D. F.R.S.; Captain W. Ramsay, R.N.; Edward Riddle, Esq.; Richard W. Rothman, Esq.; Rev. Richard Sheepshanks, M.A. F.R.S.; Lieut. William S. Stratford, R.N. F.R.S.; Charles B. Vignoles, Esq.